

importance. The space devoted to the other "Cata-racts" of the world is small, though most of the important ones are mentioned. The illustrations are good, and on the whole the book is interesting.

LETTERS TO THE EDITOR

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[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Priestley and Lavoisier

IF Mr. Rodwell had anything new to tell us about Lavoisier, there would have been a sufficient motive for his writing; but I do not see what useful purpose is gained by telling us what was already known, namely, that a century ago Lavoisier rendered many important services to science; or, what was not so well known, namely, that chemistry is a French science; or, that Lavoisier was "the most generous of men," "incapable of any meanness." The real question Mr. Rodwell himself asks:—"Upon what authority does Dr. Thomson assert that Dr. Priestley informs us that he prepared the gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774?"

Mr. Rodwell quotes from Thomson's notice of Priestley; had he turned to that of Lavoisier (p. 105, vol. ii. 1831, not 1830), he would have found an answer:—"Dr. Priestley discovered oxygen in August, 1774, and he informs us in his life [this ought to be "Life," i.e. autobiography] that in the autumn of that year he went to Paris and exhibited to Lavoisier, in his own laboratory, the mode of obtaining oxygen gas by heating red oxide of mercury in a gun-barrel, and the properties by which this gas is distinguished; indeed, the very properties which Lavoisier himself enumerates in his paper. [*Mem. Acad.* 1775, pub. 1778.] There can therefore be no doubt that Lavoisier was acquainted with oxygen gas in 1774, and that he owed his knowledge of it to Dr. Priestley."

Dr. Black complained of the publication of Lavoisier's papers without any allusion whatever to what he himself had previously done on the same subject. Cavendish complained of something more than a similar neglect. The facts, as stated in Dr. George Wilson's "Life of Cavendish," are briefly these:—Blagden went to Paris in June, 1783, and informed Lavoisier of the discovery of the composition of water. Lavoisier was incredulous, expressing his opinion that the union of the two gases (O and H) would produce, not water, but an acid. Nevertheless he repeated Cavendish's experiment on a large scale; and in his account of it to the Academy on June 25, stated that the conclusion as to the compound nature of water was drawn by Laplace and himself. The charge brought against Lavoisier by Cavendish, Blagden, and Watt, was summed up by Watt to this effect, that after Lavoisier had had the theory of the composition of water explained to him, "he invented it himself."

Mr. Rodwell "utterly denies" that the acceptance of Lavoisier's doctrine was mainly due to Cavendish's discovery. A strong objection to the oxygen theory was advanced by Berthollet and others, founded on the observation that in the action of dilute acids on metals inflammable air is produced. [The inflammable air of Cavendish, in 1766, was referred not to water, but to the metals]. Whence came this element? The discovery of the composition of water answered the objection, and converted it, as Dr. Whewell remarks, into an argument in favour of the theory.

My statement that "the compound is always equal to the sum of its elements" was already known, was elicited by a remark of Lavoisier's, quoted by Mr. Rodwell:—"I am obliged in this reasoning to suppose that the weight of the bodies employed was the same after the observation as before." My statement is new to Mr. Rodwell, and he calls for references. Many of the old writers on the idea of substance acknowledged the proposition, and its best application was Wenzel's doctrine of definite and reciprocal proportions, although its full significance did not become apparent until the aeriform elements were also taken into account.

But to return to Priestley, I am bound to admit that 1744

is a mistake into which I was misled by Whewell ("His. Ind. Sci., 1857, iii. 110), who gives that date.

Priestley was presented with the Royal Society's Copley medal, as an honourable testimony to his numerous scientific discoveries, which, considering the crude state of chemistry in his time, must be regarded as admirable. He was afterwards driven from the Royal Society and from his country, his house was pillaged, and his library, manuscripts, and apparatus destroyed, and all this persecution was on account of certain opinions which happily are now widely spread. The statue at Birmingham is a less impressive tribute to his memory than the maintenance of respect for his fame; and it is with no unfriendly feeling towards Mr. Rodwell that I express an opinion that this old quarrel between Lavoisier, Priestley, and Cavendish had better be left to repose in the history of science, where it has been discussed with sufficient fulness and fairness by such writers as Thomson, Brande, Whewell, and George Wilson.

Highgate, N., December 4

C. TOMLINSON

The Forth Bridge

IN some remarks made in NATURE, vol. xxvii. p. 101, by Mr. Charles Shaler Smith, the following passage occurs:—"The tests of the last few years show conclusively, that iron exposed to compression within its buckling limit is compacted in texture and strengthened by such use while, if subjected to continuous tension beyond two-thirds of its elastic limit, it is attenuated and weakened." As I think that the words above quoted may perhaps to a certain extent mislead those who have not themselves made experiments on the elasticity of iron and steel, and on the alteration of density which can be produced by compression or extension, I would observe:—

1. That the increase of density which can be produced by compressing within the buckling-limit such rods as may be employed in the construction of bridges, would certainly not account for the strengthening of iron exposed to continuous compression. I have examined carefully the alteration of density which can be effected in iron and steel wires by longitudinal extension, &c., and even in cases where the wire was strained to breakage, and the permanent extension exceeded 20 per cent., there was no diminution of density equal to 1 per cent. Of course the words "compacted texture" may not mean that the density is increased, but the idea seems to be not uncommon among engineers, that increase of strength necessarily implies increase of density. Though I cannot at this moment lay hands upon it, I remember reading an account of some theories advanced respecting the hardening of steel, from which it was evident that the author of these theories assumed that the hardening is attended with increase of density, whereas the density of steel can be more diminished by this process than by any mechanical means with which I am acquainted.

2. It is quite true that iron, if subjected to continuous tension beyond two-thirds of its elastic limit, is attenuated, but whether the attenuation is attended with weakening or not depends largely upon the manner in which the tension is applied. If the latter is increased by small amounts at a time, and each amount allowed to act for a few hours before any increase of stress is made, not only is there comparatively small permanent extension, but there may be an actual increase of strength as far as resistance to extension is concerned. The fact is that whether we subject iron and steel to long-continued compression or extension, we increase the resistance to compression and extension respectively, mainly for the same reason, namely, that we give time for the molecules of the metal to take up such positions as will enable them to offer the maximum resistance. Thus I have proved that the value of "Young's Modulus" is considerably increased in the case of an iron wire which has suffered permanent extension, by allowing the wire to rest for some hours either loaded or unloaded; this increase of elasticity is not attended with any appreciable increase of density.

As I feel that too much precaution cannot be taken in a question of this kind, where life is at stake, I would venture to make the following suggestion:—That bars or rods of steel and iron which run the slightest risk of having at any time to undergo a considerable extending or compressing stress should before use be subjected, if possible, to the same kind of stress gradually increased in amount with intervals of some hours between each increase until a stress equal to at least three-fourths of the breaking-stress be reached. Three or four days would suffice to bring the metal to its maximum strength, both as regards resistance to permanent and to temporary strain.

Should any one be interested in the subject, I would refer them to the experiments of Mr. J. T. Bottomley (*Proc. R. Soc.*, No. 197, 1879), of Prof. Ewing (*Proc. R. Soc.*, 1880, June 10), and of myself ("Influence of Stress and Strain on the Action of Physical Forces," *Phil. Trans.*, 1882, second volume).

HERBERT TOMLINSON

King's College, Strand, December 4

Intra-Mercurial Planets—Prof. Stewart's 24⁰11d. Period, Leverrier's and Gaillois's 24⁰25d., and Leverrier's 33⁰0225d. Sidereal Periods Considered

As your regular monthly numbers did not reach our Free Library from September, 1881, until comparatively recently, and I was absent from home when they did arrive, it was only quite lately that I had an opportunity of seeing Prof. Balfour Stewart's very interesting paper "On the Possibility of Intra-Mercurial Planets," read at last year's meeting of the British Association, and published at length in your issue of September 15, 1881. "The possibility" has been almost an admitted fact for over a century, but Prof. Stewart's valuable paper discusses the relation of certain sun-spot periods to a probable sidereal period, approximately at least, of an intra-Mercurial planet of 24⁰11d.

On looking through your subsequent numbers, I was rather surprised that so suggestive a paper had not elicited quite a discussion, although it is true that Prof. Stewart remarked that "the test was not yet complete," and many may have waited to see the final results, which have not yet appeared, but perhaps will be forthcoming at the next meeting of the British Association. But the first point that struck me, although not referred to by Prof. Stewart, was the near approximation periods of 24⁰11d. days affords to Leverrier's and Gaillois's period of 24⁰25d. days noticed in your columns of August 22, 1878, which M. Gaillois endeavoured to fit to Prof. Watson's observation, in Wyoming State, of a supposed intra-Mercurial planet at 2° 9' from the Sun, during the total eclipse, July 29, 1878. M. Gaillois's difficulty seemed to be to reconcile Leverrier's formula with Prof. Watson's reasonable belief that he saw the planet in the superior part of the orbit, while Gaillois made the formula and interval require it to be in the inferior part of the orbit July 29, 1878. The only interval that Gaillois referred to was from 1750 (January 1, I presume); it might have been obvious, therefore, that quite a small fractional difference in each of so many revolutions would suffice to make the period accord with either condition that Prof. Watson's observation required; namely, *that the planet was seen at 2° 9' from superior conjunction, or 2° 9' past inferior conjunction.* For instance, I have obtained these two results for the synodical periods. The same interval for both about 46962½ days, requiring 1808½ revolutions of 25⁰96825104d., each ½ being equal to 11⁰90211506d.; that accords very closely with Prof. Watson's belief, while 1808½ revolutions of 25⁰9742355d. each, and 1808226d. remainder, meet the condition of its being 2° 9' past the inferior conjunction. Of course, as a matter of opinion, I presume it would be impossible to see the planet so near its inferior conjunction during any total eclipse of the Sun, the planet's crescent being altogether too fine. These results are simply what the conditions require in relation to approximate 26-day apparent-periods, but we must avoid exactly 26 days, or the interval would put the planet at *its elongation*, perhaps apparently 10° from the Sun, and if we tried the Lescarbault interval, from March 26, 1859, 7065½ days, singularly enough it would put the planet *in the other elongation*. Fractional differences are of course very important therefore. And I do not find either that M. Gaillois's figures 24⁰25d. for the sidereal time, and 14⁰8462 for the diurnal motion exactly accord, and neither fills the conditions required by Prof. Watson's observation, if I am approximately correct, which I think I am. For instance, 14⁰8462 diurnal motion, gives us 24⁰24862928d. for the sidereal periods, not 24⁰25d., and the synodical period would be 25⁰9729903466d., and the planet's position would be about 46° 12' in its orbit past inferior conjunction, or apparently about 8° 24' from the Sun, and 46° 12' would be about 3½ days.

The sidereal period of 24⁰25 days, makes the diurnal motion 14⁰8453608247, and puts the planet at about 6° 8' past inferior conjunction, or apparently less than 1°. The synodical revolutions would be 25⁰974562624d. and fractional remainder 0188945, or 11h. 46m. 43s., which of course would be too close to the sun. But the sidereal period and the diurnal motion should both agree, instead of producing such a difference as I have here

indicated, of nearly 45° in the revolutions. But although I believe we cannot accept the exact published figures, 24⁰25d., or 14⁰8462, still I have shown how near we may make the final results conform to them.

Adopting the same number of synodical revolutions, and practically making the best use of the formula, obtaining 1808½ revolutions, and 1808½. The revolution being 25⁰96825104d., or 25⁰9742355d., and the remainders 11⁰90211506d., or 108226. Reduced to clock time they stand as follows: 1808½ being equal to 25d. 23h. 14m. 16⁰9s. each, and 11d. 23h. 39m. 27⁰7s. remainder, and 1808½ being equal to 25d. 23h. 22m. 54s. each, and 1d. 1h. 58m. 27½s. remainder. *The latter is almost absolutely identical* with the periods that would fit the Fritsch and Stark interval from October 10, 1802, to October 9, 1819, 6208 days, or 239 periods of 25d. 23h. 23m. 51s. And from Stark to Lescarbault makes 14,413 days, which would require 555 periods of 25d. 23h. 15m. 53½s., *which affords almost exact identity* with the general mean, placing Prof. Watson's observation in the superior part of the orbit. Thus, then, we have almost positive assurance that Fritsch, Stark, Lescarbault, and Prof. Watson's planet were identical, and that Prof. Watson was correct about it being 2° 9' from superior conjunction: these interesting facts, giving a record to Lescarbault's planet of 80 years from Fritsch's observance October 10, 1802, to October next. What other "myths" will stand such satisfactory results? I am afraid that Prof. Proctor and some other astronomers have not given the attention to this question that it deserves. But there are a few exceptions deserving credit: M. de la Baume, in Paris, was engaged last year in a classification of reported observed transits, although he did not then draw any inferences respecting apparent revolutions. He regarded Fritsch, De Cuppe, Lescarbault, and Lummis' transits as the same planets, agreeing relatively with the nodes. While Lichtenburg, November 19, 1762, Hoffman, about May 10, 1764, Scott, June 28, 1847, Ritter and Schmidt, June 11, 1855, and W. G. Wright, of San Bernardino, California, October 24, 1876, whose transit was illustrated in the *Scientific American* of November 18, 1876, he regarded as another larger planet than Lescarbault's.

Adopting the same principle with Prof. Stewart's hypothetical sidereal periods of 24⁰11d., I first find what results that gives, as applied to the same interval from January 1, 1750, and then take the nearest modification I can to the conditions of Prof. Watson's observation. 360° divided by 24⁰11d. gives us 14⁰99312818 for the planet's diurnal motion, which, multiplied by 46,962½ days, gives us 704114⁰78215325, from which, subtracting the earth's motion, 46288⁰463941, leaves a residue of 657826⁰318213, which, divided by 360° gives us 1827⁰29533 synodical revolutions; using that to divide the 46962½ days, we obtain the synodical periods of 25⁰700553d. The fractional revolution .29533 is equal to 106° 19' 17", or 7d. 14h. 10m. Now while that would put the planet in the superior part of the orbit, it would still be nearly 60° from where Prof. Watson observed it. I ought to have explained before that 2° 9', or 2° 10' apparently, is about equivalent to 15° from superior conjunction, or 15° past inferior conjunction in the planet's orbit; 15° from 180°, therefore, leaves 165° as the required position, instead of 106° 19' 17". Perhaps I am only approximately correct, but sufficient for illustration. It is very evident, however, that a very slight modification of Prof. Stewart's inferential sidereal periods, 24⁰11d., would give us the 60° more required, or exact accord with Prof. Watson's observation, and the evidence would be rather in favour of 1827½ revolutions, obtained from such a solar analogy, and may still have an incidental bearing or relation to the Leverrier-Gaillois formula I have construed to require 1808½ or 1808½ revolutions. 1827½ revolutions would give us 25⁰698260334253d. for the synodical periods, and a remainder of 11⁰77836931986d. Reduced to clock time, that would give us 25d. 16h. 45m. 29⁰7s. for each apparent revolution, and 11d. 18h. 40m. 51⁰11s. for the remainder. It must be understood that these definitions, 1808½ and 1827½, with their results, are intended only to express possible general mean periods of apparent revolutions, and may not exactly apply to any of the intervals between the long list of recorded observations of supposed transits. When Leverrier, October 1876, had strong faith in sidereal periods of 33⁰0225d., it was probably a general mean from January 1, 1750, to Lescarbault, March 26, 1859, and only approximately fitted Lummis, De Cuppis, Stark, Fritsch, and others, but still in a general sense applied to some of them, while Leverrier was led to predi-